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cance of things; the ghost of the unknowable, however, is once and for all effectually laid.

My conclusion, therefore, is that *if* human reason has attained to any knowledge of reality at all, it is impossible to set any limit to its possibilities. Either complete nescience must be the outcome of our system or the road to omniscience must be left open.

J. E. CREIGHTON.

THE MOTOR POWER OF IDEAS.

The study published from the Harvard Laboratory under the above title comes to the public under conspicuous circumstances. Our New Psychology has been styled "barren and scholastic" by enthusiastic members of its own ranks, who have proposed "more progressive methods." Others have looked with anxiety upon any eagerness which should prove ill considered. Particularly the "new methods" were championed by the new laboratory at Harvard. Their first year's trial there might be regarded as preliminary, but the present report is a matured product. As a test of the "new spirit," therefore, it is an important event in science, and it is from this standpoint of consideration that this criticism is written.

I shall confine myself to the first of the five studies presented, because the sort of detailed analysis required would fill too much space if extended to them all.

The chief fact reported in Study A is, that our eye-muscles show the effect of certain central motor impulses which are produced by different optical stimuli and their immediate associations. The authors declare that this fact affords a "new method" of studying the motor power of such mental processes as observing letters, words, colors, pictures, etc., and of doing so with a fineness of discrimination which can be in no other way attained.

Now, to the ingenuity of this "new method" I pay at once such tribute as it deserves. With the authors I heartily welcome any "method," or any "spirit" which shall lay open this very fertile field. And as heartily I declare that this "new method" does promise, if properly handled, to open up many important possibilities. But grateful as we are for its promise, it is the actual results of the "new spirit" and its way of handling fertile suggestions that the scientific public has looked forward to with chief concern, and which most challenges our present consideration.

With this in mind, I will first examine the exclusiveness asserted of this "new method." The highest science is sure to be appreciative. It must be so in order to reap full advantage from previous achievements in the line of its selected problems. Especially should it be so where, as in the present case, the experimentation is both an original research and a part of the regular system of instruction in our largest school of psychology. It is of importance, therefore, that the repeated declarations of Study A should justify themselves, namely, the declarations that the new method gives valuable "answers" as to the general relation between ideas and movements, while other methods have given only "general hints," and that previously a qualitative study of the motor power of such ideas "seemed impossible."

If any one will turn to so familiar a text-book as Professor James's Psychology he will find the general subject of Study A set forth at some length. In vol. II, p. 379, under Effects on Voluntary Muscles he will read as follows:

> Every sensorial stimulus . . . innervates the muscles generally. . . . The dynamogenic value (motor power) of simple *musical tones* seems to be proportional to their loudness and height. When the notes are compounded into sad strains, the muscular strength diminishes. If the strains are gay, it is increased. The dynamogenic value of *colored lights* varies with the color. In a subject whose normal strength was expressed by 23, it became 24 when a blue light was thrown on the eyes, 28 from green, 30 for yellow, 35 from orange, and 42 from red, etc., etc.

— the report going on to give similar account of odors, tastes, pain, tobacco, alcohol, and a much wider range of stimuli than was used in Study A.

It would seem, then, that the same sort of general results as are reported in the Study had already been somewhat more widely studied by previous methods. Reading further the same text-book account, we find at least a dozen different methods which have been successfully used for the same general purposes. Such are the circulation methods of Haller and Mosso; the respiration methods of Feré and of Danilewsky; the sweat-gland method of Tarchanoff; the eye-pupil method of Sanders; the bladder methods of Mosso, of Pellicani, and of Feré; the muscle method of Feré; the knee-jerk methods of Bowditch, Mitchell, Lombard, etc., etc. Certainly here is no lack of number or of variety of methods open to the study of the Motor Power of Ideas of the same general range as were experimented upon in the "new method."

But perhaps the "new method" is better than any of the former ones. Of course this should not be decided without discussion. Personally I think that several of the above methods could have been made to serve in measuring the Motor Power of observing letters, words, colors, etc., equally well with the "new method," and one of them, I am confident, would have served in every respect much better. I refer to the common knee-jerk method of Professors Bowditch and Lombard. There is not one of the mental occupations used in Study A to which it is not markedly sensitive. Its 'curves' are visible, continuous, vigorous, and mechanically accurate to any desired degree — far more accurate than guessing at the coincidence between the line of sight and the figures on the measuring arc used in Study A. The harness is easily put on and worked. By Professor Lombard it is kept on all night while the curve goes on continuously registering every slight disturbance of slumber, and the very ups and downs in the motor power of our dreams. Above all, the knee-jerk method is free of the very faults which, as we shall presently discuss, make the success of the new method most doubtful. For it enables us, when they are undesirable, to be free of volitional head movements and like complications, and permits a large number or variety of these when their coöperation is desirable.

In truth, then, it must be said that the Study is scarcely as fruitful of *exclusively* new results and methods as its enthusiasm leads the reader to infer. Yet if this were its main feature, most surely my Discussion would never have been written. From the first I have had chiefly at heart to compare the new modes of conducting psychological inquiry with former methods of scientific investigation in general. To this end, therefore, let us now look more closely at the general reasonings of the Study, and to its way of handling its problems altogether.

In summarizing its new method the Study declares as follows (p. 443):

> When I open my eyes and see a flame, this optical impression brings out the motor effect which is the essential element in attention. When I turn my head with closed eyes, this head movement is naturally coördinated with the eye movements, and the head movement acts as stimulus to a motor reaction of the eyes. This stimulus is, of course, the same, whether I turn my head after twenty seconds or after one second. Now if this same stimulus brings out so very different effects, it must be because the stimulus from the optical impression is different after twenty seconds and after one second. *After one second the optical stimulus is stronger than*

> *the head stimulus, and the eyes turn therefore to the flame; after twenty seconds the motor power of the optical stimulus is fully discharged, or at least so exhausted that it is overpowered by the head stimulus, and the eyes follow the head.* . . . The question arises, What is the effect if we take other optical objects instead of the flame? Is the motor power changed if I change the color of the flame, or if I take a picture, or a figure, or words, and what influence has the character of these objects? If we open the eyes after turning the head, and state in what direction our eyes are looking, while at the same time measuring the position of the head, we shall then be able to measure the motor power of the optical impression by its dependence both upon the time we look upon the objects and upon the character of the objects themselves. In this way we developed the systematic method upon which the following experiments were worked out.

This is the sort of reasoning upon which the experiments were worked out. Let us therefore examine what sort of reasoning this is. First we will be sure about the eye movements. The eyes were both carried by the turning of the head, and also they turned in the head. The latter unquestionably is the thing to be inquired about. This is the movement that measured the Motor Power of Ideas. The crucial facts are: Before turning the head the eyes were in one position of adjustment, and after the head had turned they were found to be in a different position of adjustment. Now what does the Study say caused this change in adjustment? In the lines which I have put in italics the authors declare that an "optical stimulus" and "a head stimulus" are responsible for the change. Throughout the Study no third factor is once referred to. The "optical impression" and the "head stimulus," it is declared, work in contrary directions; when the former is strongest the eyes "turn *therefore* toward the flame," when it is "overpowered by the head stimulus, the eyes follow the head."

I will first examine the "head stimulus." The authors tell us that "this head movement is naturally coördinated with the eye movements." When, therefore, they say that "the eyes follow the head," they must mean not merely that without opposition they are carried forward by the head, but that by reason of the head stimulus the eyes turn *in* the head in the direction the head moves; or at least tend to turn in that direction proportionally as the head stimulus "overcomes" the optical stimulus. The report is abundantly vague, but the only interpretation of the authors' assertion that "the head movement acts as stimulus to *a motor reaction* of the eyes" is the one I have given. Their words mean this or nothing. But what evidence do the authors give that the "head stimulus does thus turn the eyes in opposition to the optical impression"? Absolutely none whatever. They assume this outright, without experiment of their own, or of any one else. We are obliged, therefore, to ask what *a priori* ground there is for the assumption. Again we must say, absolutely none. If for anything, there is *a priori* ground for suspecting that, through inertia, if for nothing else, the eyes would naturally lag behind rather than overshoot the mark. In this case the head stimulus would work to turn the eyes relatively *toward* the flame and not from it. Moreover, from other reasons far more intimate and crucial to the analysis of this experiment, the head stimulus *could* work *toward* the flame. I shall presently consider these. But at present I will only note the facts, first, that the first assumption in the Study is purely and wholly a fabrication which may or may not prove true, but which has the *a priori* suspicion rather against it than in its favor; and second, that this assumption was made without experiment or inquiry.

I will next examine the "optical impressions." The authors' assumption is, that the "optical impression" which was received before closing the eyes, persisted after closing them, and when the head was turned, worked upon the adjustment of the eyes in a way to turn them in the head in the direction backwards from the movement of the head. Of course no one looks upon this "optical impression" as an "eye-bolt and hawser" hooked outside of the eye and holding it back. It must work internally through the ocular motor nerves. But here, under the authors' assumption, arises a great difficulty. If we conceive that a given optical impression, which we will call x, has worked a given adjustment of the eyes, which we will call A^1, it is impossible to conceive that the mere persistence of *this* impression alone and *unchanged* should cause a *change* in adjustment — let us say to A^2. Even if the head stimulus did work to change the adjustment A^1, it cannot be assumed without proof or argument that the continuance of x worked otherwise than *to preserve* the adjustment that it had normally occasioned. In short, the only *a priori* assumption logically possible is, that the optical impression did work to keep the eyes in the middle of the head. But what evidence or argument do the authors offer to the contrary or that the optical impression did or could turn the eyes toward the flame? Again, as with the head stimulus, none whatever.

These, then, are the facts of the case. Undoubtedly the eyes moved. The *a priori* suspicion is that the head stimulus caused the eyes to turn back and the optical impression worked to oppose this movement. The experimenters assume the reverse of this throughout, and do so without the least iota of fact, or the least shadow of hypothesis put forward in excuse for doing so.

At once we must declare that this manner of conducting a problem is scarcely scientific. The full responsibility for it, however, can be estimated only after inquiring about it further. I do not propose to answer the question, What did make the eyes turn back? That can only be determined by scientific investigation. But out of several possible answers I will suggest one.

When we contemplate an object "face on," we have a sense of its position with reference to self and to space in general, which we may sum up as "*a sense that it is there.*" After turning the face "side to," we have a new sense of position, a sense "*that it is now back there.*" I have no doubt that the first "optical impression" plays a part in both mental conditions. But it must be observed that it plays but an indeterminate part even in the first. A sense of bodily movement, gained by life-long acquirement, and of the spatial arrangements of the room and its furniture, gained by weeks of familiarity, cannot be uncritically summed up under "optical impression" of a "word" or "figure." Much less can the second state be classed as "optical impression." Very plainly what happened here, after the eyes were closed, in order to constitute the state "the object is now back there" is far more bodily and general experience than passing optical impression. But if the first total state which worked the adjustment A^1 be called x, and the second state be called $b\,x$, we may well conceive that it was b which caused the eyes to turn back toward the flame, and not x at all. We have said that x *ought* to work to the continuance of A^1. On the other hand, b is the kinesthetic product of the head movement, the memory of the moving, and is closely associated with the moving. What more natural, then, the moment the head begins to turn, than that these elements constituting the notion "of *back there from the new position*" begin to rise and to work their customary effect, namely, to turn the eyes "back there"? In this case the motor power causing the eye movement need not be the optical impression in any degree whatever.

I do not offer this hypothesis as a final solution, but certainly it is more plausible than that x alone can work both A^1 and A^2. And if so, what now must we conclude as to the authors' failure to consider such a possibility? We might, here, let them speak out of their own mouths. They rejected more than half of their subjects because the volitional impulse for turning the head with closed eyes "*has upon them the same effect as if they saw an object in indirect vision and in order to look at it directly, made a head movement.*" I could not better describe my above hypothesis: my *b*, if it exists, is a memory of *seeing* an object in indirect vision, a memory of the act, not of the object, and works to turn the eye accordingly. And if the reason given by the authors is valid for throwing out half of their subjects, it is difficult to see why they must not all be thrown out. Nor must we be blinded by the fact that the authors were fearing alone the effects of "visualizing." What they really feared was, that if some other mental state than the optical impression received before closing the eyes, should be directly accountable for the eye movement, then it could not be claimed that this eye movement was a measurement of the "motor power" of the optical impression. Personally, I see no reason why any of the subjects should have been thrown out, and only touch upon the matter to show the perversity of the authors' reasoning. So far as I can see, the results show that different kinds of very complex mental occupations affect differently some *unknown* tendency to move the eyes. And to throw out half of the results because they included visualizing tendencies seems like introducing a degree of refinement, altogether too scarce in the experiment, at the very wrong point. If our hypothesis be correct, the "optical impression," would have *all the more* effect proportionally as the subjects did visualize, and doubtfully any at all unless they did. That is, unless *b* contained some optical elements common to the "optical impression," the latter would be all the less concerned in the "motor power" of the eye movement.

The Study purports to give "answers" to certain important psychological problems. These answers were deduced from the data of the experiments under the interpretation that when the eye-movements were great the motor power of the "optical impression" should be proportionally great. From an *a priori* point of view, should our above suppositions prove correct, the interpretations and answers given in the Study would be precisely reversed. Yet I am not prepared to say that either of these interpretations would be the right one. The problem concerned is far more complicated than the Study gives intimation of. If any one will but read the literature of these experiments, which the Study passes over as giving us only "general hints," he will find that the problem has already been carried somewhat further than the conception of a simple and constant relation between cerebral occupations and movements generally. Should he read no further than the text-book account already quoted from, he will discover in a foot-note the following: "Professor H. P. Bowditch has made the interesting discovery that if the reinforcing movement (the homologue of the optical impression of Study A) be as much as 0.4 of a second later, the reinforcement fails to occur, and is transformed into a positive inhibition." I have not space to draw all the analogies implied here, but any one conversant with the subject will at once understand that, translated into terms of Study A, the above note suggests that the time relations between the several stimuli, whatever they may prove to be, which govern the eye-movements, may alone and of themselves, under different conditions, turn the motor effects from positive to negative phenomena. Not only this, but we will not have to study long to discover that all mental occupations do not work the same motor effects on the lower centres. Nothing is better known than the fact that certain thoughts hinder certain actions, while other thoughts and actions work in harmony. These matters have already been taken account of in the development of the knee-jerk method, and, as a consequence, some extremely valuable problems of Will and of Attention have been opened to experimentation, and are now being worked upon along these lines by different investigators; all of which, however, Study A appears to entirely ignore. In face of all this, I will not attempt to say what should be the proper interpretation of the figures presented from the Harvard experiments, but surely it should make one hesitate about assuming outright that the after-effects of every sort of cerebral occupation indiscriminately give positive motor innervations, or that they follow simple and invariable laws with reference to time element. The fact that the majority of the figures in the Study — those from two out of three subjects worked upon — contradict the simple interpretation deduced from them in the report, conspicuously emphasizes, if I mistake not, the fault of not considering these broader complications of the problem. As a consequence, the inexperienced reader of Study A is, on the one hand, left with the impression that fixed and definite answers are given him, by the "new method," regarding matters never before subjected to exact determination, while, on the other hand, he is left quite unaware of several difficulties and achievements already developed within his problem, and of a wide range of resources and methods already open to his service.

In the major matters of conducting the problem and of handling and presenting the results, we find much, therefore, in Study A which does not command hearty admiration of our "new psychological spirit." If we descend to minor details, we find other things difficult to reconcile with what heretofore have been considered scientific canons. For instance: I have already said that the authors threw out half of their subjects because they visualized the direction of the head movement. Passing over any criticism of whether they should be rejected or not if they did visualize, we will ask: How did the experimenters determine that the subjects should be thrown out or not? Did they merely *ask* the subjects if they visualized, and take their word for it? An old-fashioned experimenter would have felt bound to give an account of his test for what he declared to be a crucial point in his experimentation. The Study ignores the subject completely.

Again, it was a canon of Experimental Psychology heretofore, that in delicate problems, the subjects who were operated upon should be free from any intimation of what was expected to happen. It is well known that such intimations often act like hypnotic suggestions to determine what shall happen, and through habit soon become chronic. Now in Study A the experiences of the earlier "physicist" were confessedly in the minds of the experimenters from the first, and it may be that the turning back of the eyes was due to the suggestion that they would turn back — to continued imitation, as Professor Royce would say, of the notion of turning back. Certainly, control-experiments upon subjects unaware of such expectations were extremely desirable to provide against such possibility.

Not less than in other particulars does the Study offend by gross literary carelessness. Brief, every scientific report should be. But in proportion to its brevity should it be free of misleading suggestions. Of these, this one short Study contains several. A sample is as follows: On p. 443, in summarizing "the way we developed the systematic method," the crucial point is made that "the stimulus from the optical impression is different after twenty seconds and after one second." Unmistakably the intimation here is that the motor power of the impression fades with lapse of time. "After twenty seconds the motor power is fully discharged." "What, then, was its strength during this interval? What after two, or three, or ten seconds?" How are we surprised, therefore, to be told on p. 445 that "Our special question was, how the motor impulse to fixate the eyes upon an optical object varies with the quality of the impression and with the time of fixation, especially with regard to the intervals of 1, 2, 3, 4 seconds." Can it be possible that the authors distinguish no difference between mere lapse of time *after the impulse is received*, and the lapse of time *during* which it *is* received? As a matter of fact, they theorize about the first proposition — the lapse after the impression — and experiment regarding the second proposition, *i.e.*, the 1, 2, 3, 4 seconds in their tables refer to the length of time that the stimulations were continued. Not a single word of warning is given for the change from one proposition to the other. Such confusion and carelessness is inexcusable, even from a literary point of view, and leaving out of consideration deeper scientific complications which I have no time here to untangle.

Further evidence of similar carelessness is found in the author's use of such terms as "head stimulus" and "optical stimulus." Throughout, the vast complication of processes lying between the first retinal attack and the final eye-movements is simply "lumped," and now referred to by one term and now by another. What part of the total "motor power" is "ideas," and what "reflex coördination"; what is the "sight" of figures, and what the "adding" of figures"; what is the "essential factor" of attention, and what the "complicated emotional state" of attention — all these particulars are indistinguishably swamped in the vague and vast ocean of generalities. Under such generalities one could as well claim that the time occupied in emptying a theatre, after a cry of "fire," would be a scientific measurement of "the motor power of ideas." Even the most meagre analysis should discover at least four factors in the Study's problem — the retinal influences, their immediate cerebral associations, the volition to turn the head and its coördinated effects on the eye-movements, and the automatic conditions and processes of the ocular motor centres. The Study, however, knows of nothing but the "optical impressions" and the "head stimulus." It fails utterly to recognize any difference between the part of the optical stimulus which acts upward to awaken ideas, and the other part that acts downward, automatically adjusting the eyes. It fails utterly, also, to discover that the "head stimulus" is no simple matter, but one likely, as I have already shown, to involve impressions of "looking back there" and of bodily positions of the greatest intricacy. It is only through vagueness of this kind that the delusion could be sustained that the same "optical impression" could be the cause both of the positive movements of the eyes, and at the same time of the negative variations displayed in those movements.

At this point it may well be asked, What would have been expected in the conduct of such a problem as was here presented, from the old school of scientific investigation? Starting from the general suggestion that some sort of influence causes the eyes to turn back, and that this movement might be utilized for studying the motor effects of various mental occupations, we may reasonably suppose that an old-school scientist would at once have recognized that the suggestion offered him two distinct problems: namely, the motor power of ideas, and the cause of the eyes turning back. Noting these two problems, if he had then determined to study the motor power of ideas, it seems to me doubtful if he would have proceeded by the eye-method at all; that he might have hesitated to do so for the reason that the knee-jerk method offers so many obvious advantages for that particular work. But in case he should decide to develop the eye-method, then we cannot conceive it possible that he would have brought it before the public in the condition in which Study A presents it. He would at least have waited in order to make control-experiments for determining both how the eyes behave normally when turning the head, *i.e.*, without previous fixation upon any object, and also how they behave when turned after fixation, the head remaining still. Also, he would have waited until he had analyzed his problem to discover what other factors besides "head stimulus" and "optical stimulus" might enter into it, and under what conditions. Above all, he would have mastered the previous literature of his problem, and, at least, until he had made positive contributions going markedly beyond his predecessors, he would have been generously appreciative of prior achievements.

In summary, then, what shall we say of the "New Spirit," according to which it is proposed to conduct future psychological investigation? Is it fertile of important and permanent results above previous "hints" and methods which it despises? Does it clear the general atmosphere of its problems, or rather leave them in greater confusion? Does it display that carefulness of thought and sobriety of statement which is calculated to raise our science out of the suspicion with which it is frequently regarded by the rigid disciplinarians of the other sciences? Is it calculated to inspire confidence among thorough students of any profession? Is it well adapted to the training of young students? Is it replete with suggestions for them as to current psychological methods and achievements? Every reader must answer these questions for himself. For myself, with the deepest reverence for psychology, I am compelled to say that the new method of experimentation appears to be of extremely doubtful expediency. I cannot but feel that the young, and perhaps also the majority of professional readers, are likely rather to be misled and harmed by the loose deductions and careless declarations of the Study, than benefited by its production.

HERBERT NICHOLS.

ON THE ELENCH OF THE LIAR.

THE famous Elench of the Eubulides: if a man acknowledges that he lies, does he lie or speak the truth? is treated, as by Hegel (*History of Philosophy*, vol. I, p. 459), as a formal contradiction. Hegel remarks, "If it is said that he tells the truth, this contradicts the content of his utterance, for he confesses that he lies. But if it is asserted that he lies, it may be objected that his confession is the truth. He thus both lies and does not lie; but a simple answer cannot be given to the question raised. For here we have a union of two opposites, lying and truth, and their immediate contradiction."

But the difficulty of this Elench rests, I think, not, as generally supposed, on logical contradiction, but on psychological confusion. Strictly speaking, one cannot acknowledge to lying, cannot say I lie, but only I lied. A lie is always about something, hence one cannot in the same psychic act lie and acknowledge to this lie, *i.e.*, a psychic act is never its own object. In the nature of psychic fact a lie cannot take cognizance of itself. Hence the acknowledging to a lie need not be confounded with the lie acknowledged. But a man may lie in the acknowledging to a lie, he may lie about lying as about anything else; he may falsely say, "I lied." Thus he may falsely say, "I lied in telling you it was ten miles to Dover." A man may then lie in saying that he lied, and lie in saying that he lied about lying, and lie in this last acknowledgment, and so on in infinite *regressus*; but however far we go back, we do not get contradiction, but falseness or truth may be properly affirmed of any acknowledging act. The acknowledging act may at any stage be a lie, but it always takes a new acknowledging act to acknowledge the lie. Hence I take it that psychological analysis shows that the Elench can always receive a simple answer: in every acknowledging, he either lies or does not lie.

But the Elench, properly stated, is this: Can the liar confess to a lie? If he confesses truthfully, of course he is not a liar; only by claiming to be truth-teller does he become liar. However, if on the other hand, he confesses untruthfully to a lie, he implicates himself in a truth, and so is not liar. If a man falsely declares that he lied in saying it was ten miles to Dover, he says he lied when he did not lie, and thus admits himself as truth-teller. The real paradox, then, is